XIV. *On the inverted Action of the alburnous Vessels of Trees.* *By* Thomas Andrew Knight, *Esq. F. R. S. In a Letter to the Right Hon. Sir* Joseph Banks, *K. B. P. R. S.*

Read May 15, 1806.

MY DEAR SIR,

I HAVE endeavoured to prove, in several Memoirs* which you have done me the honour to lay before the Royal Society, that the fluid by which the various parts (that are annually added to trees, and herbaceous plants whose organization is similar to that of trees,) are generated, has previously circulated through their leaves† either in the same, or preceding season, and subsequently descended through their bark; and after having repeated every experiment that occurred to me, from which I suspected an unfavourable result, I am not in possession of a single fact which is not perfectly consistent with the theory I have advanced.

There is, however, one circumstance stated by HALES and

* In the Phil. Trans. for 1801, 1803, 1804, and 1805.

† During the circulation of the sap through the leaves, a transparent fluid is emitted, in the night, from pores situated on their edges; and on evaporating this liquid obtained from very luxuriant plants of the vine I found a very large residuum to remain, which was similar in external appearance to carbonate of lime. It must, however, have been evidently a very different substance from the very large portion, which the water held in solution. I do not know that this substance has been analyzed, or noticed by any naturalist

DU HAMEL which appears strongly to militate against my hypothesis; and as that circumstance probably induced HALES to deny altogether the existence of circulation in plants, and DU HAMEL to speak less decisively in favour of it than he possibly might otherwise have done, I am anxious to reconcile the statements of these great naturalists, (which I acknowledge to be perfectly correct,) with the statements and opinions I have on former occasions communicated to you.

Both HALES and DU HAMEL have proved, that when two circular incisions through the bark, round the stem of a tree, are made at a small distance from each other, and when the bark between these incisions is wholly taken away, that portion of the stem which is below the incisions through the bark continues to live, and in some degree to increase in size, though much more slowly than the parts above the incisions. They have also observed that a small elevated ridge (*bourvelet*) is formed round the lower lip of the wound in the bark, which makes some slight advances to meet the bark and wood projected, in much large quantity, from the opposite, or upper lip of the wound.

I have endeavoured, in a former Memoir,* to explain the cause why some portion of growth takes place below incisions through the bark, by supposing that a small part of the true sap, descending from the leaves, escapes downwards through the porous substance of the alburnum. Several facts stated by HALES seem favourable to this supposition; and the existence of a power in the alburnum to carry the sap in different directions, is proved in the growth of inverted cuttings of different species of trees.† But I have derived so

* Phil. Trans. for 1803. † Ibid. for 1804.

many advantages, both as a gardener and farmer, (particucularly in the management of fruit and forest trees,) from the experiments which have been the subject of my former memoirs, that I am confident much public benefit might be derived from an intimate acquaintance with the use and office of the various organs of plants; and thence feel anxious to adduce facts to prove that the conclusions I have drawn are not inconsistent with the facts stated by my great predecessors.

It has been acknowledged, I believe, by every naturalist who has written on the subject, (and the fact is indeed too obvious to be controverted,) that the matter which enters into the composition of the radicles of germinating seeds existed previously in their cotyledons; and as the radicles increase only in length by parts successively added to their apices, or points most distant from their cotyledons, it follows of necessity that the first motion of the true sap, at this period, is downwards. And as no alburnous tubes exist in the radicles of germinating seeds during the earlier periods of their growth, the sap in its descent must either pass through the bark, or the medulla. But the medulla does not apparently contain any vessels calculated to carry the descending sap; whilst the cortical vessels are during this period much distended and full of moisture: and as the medulla certainly does not carry any fluid in stems or branches of more than one year old, it can scarcely be suspected that it, at any period, conveys the whole current of the descending sap.

As the leaves grow, and enter on their office, cortical vessels, in every respect apparently similar to those which descended from the cotyledons, are found to descend from the bases of the leaves; and there appears no reason, with

which I am acquainted, to suspect that both do not carry a similar fluid, and that the course of this fluid is, in the first instance, always towards the roots.

The ascending sap, on the contrary, rises wholly through the alburnum and central vessels; for the destruction of a portion of the bark, in a circle round the tree, does not immediately in the slightest degree check the growth of its leaves and branches: but the alburnous vessels appear, from the experiments I have related in a former Paper,* and from those I shall now proceed to relate, to be also capable of an inverted action, when that becomes necessary to preserve the existence of the plant.

As soon as the leaves of the oak were nearly full grown in the last spring, I selected in several instances two poles of the same age, and springing from the same roots in a coppice, which had been felled about six years preceding; and making two circular incisions at the distance of 3 inches from each other through the bark of one of the poles on each stool, I destroyed the bark between the incisions, and thus cut off the communication between the leaves and the lower parts of the stem and roots, through the bark. Much growth, as usual, took place above the space from which the bark had been taken off, and very little below it.

Examining the state of the experiment in the succeeding winter, I found it had not succeeded according to my hopes; for a portion of the alburnum, in almost every instance, was lifeless, and almost dry, to a considerable distance below the space from which the bark had been removed. In one instance the whole of it was, however, perfectly alive; and in

* Phil. Trans. for 1804.

this I found the specific gravity of the wood above the decorticated space to be 1114, and below it 1111; and the wood of the unmutilated pole at the same distance from the ground to be 1112, each being weighed as soon as it was detached from the root.

Had the true sap in this instance wholly stagnated above the decorticated space, the specific gravity of the wood there ought to have been, according to the result of former experiments,* comparatively much greater: but I do not wish to draw any conclusion from a single experiment; and indeed I see very considerable difficulty in obtaining any very satisfactory, or decisive facts from any experiments on plants, in this case, in which the same roots and stems collect and convey the sap during the spring and summer, and retain, within themselves, that which is, during the autumn and winter, reserved to form new organs of assimilation in the succeeding spring. In the tuberous-rooted plants, the roots and stems which collect and convey the sap in one season, and those in which it is deposited and reserved for the succeeding season, are perfectly distinct organs; and from one of these, the potatoe, I obtained more interesting and decisive results.

My principal object was to prove that a fluid descends from the leaves and stem to form the tuberous roots of this plant; and that this fluid will in part escape down the alburnous substance of the stem when the continuity of the cortical vessels is interrupted: but I had also another object in view.

Every gardener knows that early varieties of the potatoe never afford either blossoms or seeds; and I attributed this

* Phil. Trans. for 1805.

peculiarity to privation of nutriment, owing to the tubers being formed preternaturally early, and thence drawing off that portion of the true sap, which in the ordinary course of nature is employed in the formation and nutrition of blossoms and seeds.

I therefore planted, in the last spring, some cuttings of a very early variety of the potatoe, which had never been known to blossom, in garden pots, having heaped the mould as high as I could above the level of the pot, and planted the portion of the root nearly at the top of it. When the plants had grown a few inches high, they were secured to strong sticks, which had been fixed erect in the pots for that purpose, and the mould was then washed away from the base of their stems by a strong current of water. Each plant was now suspended in air, and had no communication with the soil in the pots except by its fibrous roots, and as these are perfectly distinct organs from the runners which generate and feed the tuberous roots, I could readily prevent the formation of them. Efforts were soon made by every plant to generate runners and tuberous roots; but these were destroyed as soon as they became perceptible. An increased luxuriance of growth now became visible in every plant, numerous blossoms were emitted, and every blossom afforded fruit.

Conceiving, however, that a small part only of the true sap would be expended in the production of blossoms and seeds, I was anxious to discover what use nature would make of that which remained; and I therefore took effectual means to prevent the formation of tubers on any part of the plants, except the extremities of the lateral branches, those being the points most distant from the earth, in which the tubers are

naturally deposited. After an ineffective struggle of a few weeks, the plants became perfectly obedient to my wishes, and formed their tubers precisely in the places I had assigned them. Many of the joints of the plants during the experiment became enlarged and turgid; and I am much inclined to believe, that if I had totally prevented the formation of regular tubers, these joints would have acquired an organization capable of retaining life, and of affording plants in the succeeding spring.

I had another variety of the potatoe, which grew with great luxuriance, and afforded many lateral branches; and just at that period, when I had ascertained the first commencing formation of the tubers beneath the soil, I nearly detached many of these lateral branches from the principal stems, letting them remain suspended by such a portion only of alburnous and cortical fibres and vessels as were sufficient to preserve life. In this position I conceived that if their leaves and stems contained any unemployed true sap, it could not readily find its way to the tuberous roots, its passage being obstructed by the rupture of the vessels, and by gravitation; and I had soon the pleasure to see that instead of returning down the principal stem into the ground, it remained and formed small tubers at the base of the leaves of the depending branches.

The preceding facts are, I think, sufficient to prove that the fluid, from which the tuberous root of the potatoe, when growing beneath the soil, derives its component matter, exists previously either in the stems or leaves; and that it subsequently descends into the earth: and as the cortical vessels during every period of the growth of the tuber are filled with

the true sap of the plant, and as these vessels extend into the runners, which carry nutriment to the tuber, and in other instances evidently convey the true sap downwards, there appears little reason to doubt that through these vessels the tuber is naturally fed.

To ascertain, therefore, whether the tubers would continue to be fed when the passage of the true sap down the cortical vessels was interrupted, I removed a portion of bark of the width of five lines, and extending round the stems of several plants of the potatoe, close to the surface of the ground, soon after that period when the tubers were first formed. The plants continued some time in health, and during that period the tubers continued to grow, deriving their nutriment, as I conclude, from the leaves by an inverted action of the alburnous vessels. The tubers, however, by no means attained their natural size, partly owing to the declining health of the plant, and partly to the stagnation of a portion of the true sap above the decorticated space.

The fluid contained in the leaf has not, however, been proved, in any of the preceding experiments, to pass downwards through the decorticated space, and to be subsequently discharged into the bark below it: but I have proved with amputated branches of different species of trees that the water which their leaves absorb, when immersed in that fluid, will be carried downwards by the alburnum, and conveyed into a portion of bark below the decorticated space; and that the insulated bark will be preserved alive and moist during several days;* and if the moisture absorbed by a leaf can be

* This experiment does not succeed till the leaf has attained its full growth and maturity, and the alburnum of the annual shoot its perfect organization.

thus transferred, it appears extremely probable that the true sap will pass through the same channel. This power in the alburnum to carry fluids in different directions probably answers very important purposes in hot climates, where the dews are abundant and the soil very dry; for the moisture the dews afford may thus be conveyed to the extremities of the roots: and HALES has proved that the leaves absorb most when placed in humid air; and that the sap descends, either through the bark or alburnum, during the night.

If the inverted action of the alburnous vessels in the decorticated space be admitted, it is not difficult to explain the cause why some degree of growth takes place below such decorticated spaces on the stems of trees; and why a small portion of bark and wood is generated on the lower lip of the wound. A considerable portion of the descending true sap certainly stagnates above the wound, and of that which escapes into the bark below it, the greater part is probably carried towards, and into, the roots; where it preserves life, and occasions some degree of growth to take place. But a small portion of that fluid will be carried upwards by capillary attraction, between the bark and the alburnum, exclusive of the immediate action of the latter substance, and the whole of this will stagnate on the lower lip of the wound; where I conceive it generates the small portion of wood and bark, which HALES and DU HAMEL have described.

I should scarely have thought an account of the preceding experiments worth sending to you, but that many of the conclusions I have drawn in former memoirs appear, at first view, almost incompatible with the facts stated by HALES and

DU HAMEL, and that I had one fact to communicate relative to the effects produced by the stagnation of the descending sap of resinous trees, which appeared to lead to important consequences. I have in my possession a piece of a fir-tree, from which a portion of bark, extending round its whole stem, had been taken off several years before the tree was felled; and of this portion of wood one part grew above, and the other below, the decorticated space. Conceiving that, according to the theory I am endeavouring to support, the wood above the decorticated space ought to be much heavier than that below it, owing to the stagnation of the descending sap, I ascertained the specific gravity of both kinds, taking a wedge of each as nearly of the same form, as I could obtain, and I found the difference greatly more than I had anticipated, the specific gravity of the wood above the decorticated space being 0.590, and of that below only 0.491: and having steeped pieces of each, which weighed a hundred grains, during twelve hours in water, I found the latter had absorbed 69 grains, and the former only 51.

The increased solidity of the wood above the decorticated space, in this instance, must, I conceive have arisen from the stagnation of the true sap in its descent from the leaves; and therefore in felling firs, or other resinous trees, considerable advantages may be expected from stripping off a portion of their bark all round their trunks, close to the surface of the ground, about the end of May or beginning of June, in the summer preceding the autumn in which they are to be felled. For much of the resinous matter contained in the roots of these is probably carried up by the ascending sap in the

spring, and the return of a large portion of this matter to the roots would probably be prevented:* the timber I have, however, very little doubt would be much improved by standing a second year, and being then felled in the autumn; but some loss would be sustained owing to the slow growth of the trees in the second summer. The alburnum of other trees might probably be rendered more solid and durable by the same process; but the descending sap of these, being of a more fluid consistence than that of the resinous tribe, would escape through the decorticated space into the roots in much larger quantity.

It may be suspected that the increased solidity of the wood in the fir-tree I have described was confined to the part adjacent to the decorticated space; but it has been long known to gardeners, that taking off a portion of bark round the branch of a fruit-tree occasions the production of much blossom on every part of that branch in the succeeding season. The blossom in this case probably owes its existence to a stagnation of the true sap extending to the extremities of the branch above the decorticated space; and it may therefore be expected that the alburnous matter of the trunk and branches of a resinous tree will be rendered more solid by a similar operation.

* The roots of trees, though of much less diameter than their trunks and branches, probably contain much more alburnum and bark, because they are wholly without heart wood, and extend to a much greater length than the branches; and thence it may be suspected that when fir-trees are felled, their roots contain at least as much resinous matter, in a fluid moveable state, as their trunks and branches; though not so much as is contained, in a concrete state, in the heart wood of those.

I send you two specimens of the fir wood I have described, the one having been taken off above, and the other below, the decorticated space. The bark of the latter kind scarcely exceeded one-tenth of a line in thickness; the cause of which I propose to endeavour to explain in a future communication relative to the reproduction of bark.

I am, &c.

T. A. KNIGHT.